ON THE EVIDENCE THAT THE EARTH'S INTERIOR IS SOLID.

BY DR. M. E. WADSWORTH.

IT is well known that the facts of geology require some mode of escape from the now generally-received opinion that this earth is a solid globe. This is especially shown by the introduction of so many hypotheses to avoid the logical consequences incident upon such a globe. Some of these hypotheses are lakes of still unsolidified material; masses of readily fusible materials; aqueo-igneous solution of sediments; fusion of the earth's interior on the removal of pressure; a solid crust and a solid core with a zone of liquid matter between, etc., etc. All these views are simply compromises between the known fact that some part of the earth must be liquid in order to account for geological phenomena and the supposed physical and mathematical demonstrations of the earth's solidity. These compromises are unnecessary if it can be shown that the claims for the earth's solidity are not well based.

The facts of petrography likewise seem to the writer to demand the belief that all eruptive rocks have come from the earth's interior, below any of the sedimentary deposits. Also that they must have come from material that has either never been solidified or else has been reliquified.

This paper has arisen out of the fact that to the writer's mind there appeared a diversity between the commonly taught hypothesis of the earth's structure and observed petrographical facts. He therefore undertook an examination of the principal published data in order to see whether the error resided in the petrographical observations or in the data for the hypothesis of a solid earth. The result led to the writing of this paper nearly two years ago, and it now stands essentially unchanged. Its object is to place before others the chief lines of argument in behalf of a solid earth and to indicate that geology and petrography have as yet the right to assume any structure for the earth's interior consistent with known facts, without regard to the so-called physical and mathematical demonstrations of its solidity.[1]

The Evidence of the Earth's Solidity derived from Precession and Nutation, and the Tides.—The most prominent of the early advo-

[1] *Science*, 1883, I, 127–150.

cates of the essential solidity of the earth, was Mr. William Hopkins, who based his conclusion upon the phenomena of precession and nutation. In order to mathematically investigate this question, he made two assumptions regarding the earth: 1. That it was composed of a homogeneous fluid mass contained in a homogeneous solid shell. 2. A heterogeneous fluid inclosed in a heterogeneous solid shell. In both cases the transition was assumed to be "immediate between the entire solidity of the shell to the perfect fluidity of the interior mass." He further assumed that the circulation would go on in every portion of the mass until it had lost its perfect fluidity throughout the entire mass at nearly the same instant. This of course could only be correct if the liquid was homogeneous. He did, however, state that a viscid condition of the liquid would prevent the descent of the cooled exterior portions towards the interior. With a globe constituted as above for his basis, Hopkins concluded that the phenomena of precession and nutation did not demand on the earth a crust of over 1000 miles in thickness, but they did require one at least 800 miles thick. In order to account for volcanic phenomena, he assumed that there were in the solidified crust, lakes of molten material whose origin was to be ascribed to a greater fusibility of the material composing them, than of that forming the surrounding solid crust.[1]

Professor Hennessy later held[2] that in a fluid globe the denser particles would sink through the lighter, while the lighter ones would ascend through the heavier, until an equilibrium of the mass would be reached, so that the globe "would consist of a series of spheroidal strata, each of uniform density throughout its own mass." He further remarked: "The exterior portions of the fluid would cool first, until they would acquire, according to the particular circumstances which may influence their cooling, certain densities. If the effect of refrigeration be in general an increase in density of the matter cooled, then the cooled portions of the fluid will sink. The higher temperature of the matter yet unexposed to cooling influence would then tend to change the acquired densities of the cooled matter. The as yet uncooled fluid would have its temperature reduced from contact with the cold particles from above, and it would tend to change its posi-

[1] Philos. Trans., 1839, pp. 381–423; 1840, pp. 193–208; 1842, pp. 43–55.

[2] Philos. Trans., 1851, pp. 495–547.

tion in a similar manner to the portions first cooled. As each cooled portion of the fluid descends, the three following causes would therefore impede its descent:

1st. From the arrangement already indicated of the denser strata about the center of the mass, and from the nature of the law of density of the strata, each stratum into which the cooled fluid would descend, would be denser than the preceding.

2d. If the general effect of the refrigeration be to increase the density of the cooled matter, each stratum would have its density augmented by the passage through it of the cooler matter from above.

3d. The descending portions will have their densities diminished by the increase in their temperature.

With an earth thus constituted for his starting point, Hennessy concluded that its crust could not be less than eighteen miles thick nor more than 600 miles. He further claimed that owing to friction, viscidity, and pressure it was probable that the fluid interior and the solid exterior would rotate nearly as if the earth was solid from its center, thus denying the validity of Hopkins' conclusions.

Professor Samuel Haughton, starting from the assumed premise that the earth was "composed of a solid shell, having the density of the rocks at the surface and a fluid homogeneous nucleus," also having a fluid heterogeneous one, deduced 768 miles as the probable thickness in both cases.[1]

The conclusions of Hopkins were sustained by Sir William Thomson. He starts with the hypothesis that the earth is composed of a thin shell which passes abruptly into a homogeneous incompressible fluid, mobile like water, which forms the interior. He also assumes that this liquid interior is heterogeneous and viscid. Taking for his starting points globes such as these, he concludes that the earth is more rigid than a homogeneous globe of glass and probably than one of steel.

Thomson further claimed that it was demonstrable that the earth solidified from the center, because melted rock contracted on solidifying, and that hence the crust would sink to the center as soon as formed.[2]

[1] Trans. Roy. Irish Academy, 1851, XXII, 251–273.

[2] Trans. Roy. Soc. Edin., 1864, XXIII, 157–169; Phil. Mag., 1863 (4), XXV, 1–14, 149–151; Phil. Trans., 1863, pp. 573–582; Thomson and Tait's Natural Philosophy, 1867, I, 670–727; Trans. Geol. Soc. Glas., 1878, VI, 38–49.

Thomson's views were republished and reaffirmed in 1872,[1] but in 1876 he says, regarding his previous argument from precession and nutation, as the result of a conversation between himself and Professor Newcomb: "Trying to recollect what I had written on it fourteen years ago * * * my conscience smote me, and I could only stammer out that I had convinced myself that so-and-so and so-and-so, at which I had arrived by a non-mathematical short cut, were true. He hinted that viscosity might suffice to render precession and nutation the same as if the earth were rigid, and so vitiate the argument for rigidity. This I could not for a moment admit, any more than when it was first put forward by Delaunay.[2] But doubt entered my mind regarding the so-and-so and so-and-so, and I had not completed the night journey to Philadelphia, which hurried me away from our unfinished discussion, before I had convinced myself that they were grievously wrong."[3]

Thomson, however, strongly affirmed the correctness of his views regarding the rigidity of the earth as determined by the phenomena of the tides. It is permitted, since he still retains his view that the crust when formed would sink, from its weight, to the center, to think that if he would affirm the precession and nutation theory, for fourteen years, so often as he had, without giving it sufficient thought, that possibly with the very imperfect tidal data at his command, he has not looked at this question in all its bearings.

Professor Hennessy indeed in 1872 pointed out that Thomson had assumed a spheroidal homogeneous elastic shell filled with incompressible fluid, and that all the latter could claim to have proved was, "that the earth does not consist of an elastic solid envelope enclosing a mass of the ideal substance called an incompressible liquid." Hennessy also justly calls attention to the fact that Thomson's method of proving the rigidity of the earth by assuming, first, that it is a homogeneous mass of glass, and again of steel, is an argument of the same kind, as if one should attempt to prove that rapid locomotion in railway trains was impossible, on account of the shocks the passengers would

[1] *Nature*, 1872, V, 223–224, 257–259.

[2] This is an error of Thomson's, since Hennessy advanced this view in 1851, seventeen years before Delaunay. See *Nature*, 1871, III, 420; *Geol. Mag.*, 1871 (1), VIII, 216–218.

[3] Report Brit. Assoc., 1876, XLVI (Sect.), 1–12.

be subject to, by assuming as his premise that the carriages were rigidly attached to one another.[1]

Delaunay further objected to the precession and nutation theory of Hopkins and Thomson, on account of the slowness of the motion and the viscidity of the liquid interior, which would cause the earth to act as if it were a solid body.[2]

From observations made on the deflection of the plumb-line by the Himalaya mountains, Sir George B. Airy held, "that the whole of that country is floating upon a dense fluid, and that the thick mass of the lighter mountain-matter sinks deep in the fluid, and that the displacement of denser matter neutralizes almost entirely the attraction of the lofty mountains. The form of the earth is not such as would be taken by a solid structure, but such as would be taken by a fluid mass with solids floating upon it."[3]

In the case of Mr. Geo. H. Darwin's papers, it is difficult in some cases to understand exactly what he regards as the constitution of his assumed mathematical globes, and to express his ideas in non-mathematical language. His investigations appear to be made on the suppositions that the globe is a viscous non-elastic spheroid, an elastico-viscous one, and one either elastic, plastic or viscous. All these, if he is rightly understood, are considered to be homogeneous. Darwin then states that the chief practical result of this paper may be summed up by saying that it is "strongly confirmatory of the view that the earth has a very great effective rigidity," the term earth being substituted for the hypothetical globes mathematically investigated by him."[4]

In one paper he says that the word earth is used as an abbreviation for "a homogeneous rotating viscous spheroid."

In a later paper "On the stresses due to the weight of continents," he assumes that the earth is a homogeneous elastic sphere, of which two conditions are possible, one that it is incompressible, the other that it is compressible. He then proceeds to show that in an earth thus constituted a state of stress must exist, owing to the inequalities between the continents and sea floors. From the discussion of this supposed condition, he says: "It appears that if the earth be solid throughout, then at a thousand miles from the surface the material must be as strong as

[1] *Nature*, 1872, V, 288, 289.

[2] *Geol. Mag.*, 1868 (1), V, 507–511.

[3] *Nature*, 1878, XVIII, 41–44.

[4] Philos. Trans., 1880, CLXX, 1–35, 147–593.

granite. If it be fluid or gaseous inside, and the crust a thousand miles thick, that crust must be stronger than granite, and if only two or three hundred miles in thickness much stronger than granite. This conclusion is obviously strongly confirmatory of Sir William Thomson's view that the earth is solid throughout."[1]

This statement of Professor Darwin seems to me misleading, for if his paper is understood aright, he has proved nothing of the *actual earth*, but only of a *hypothetical homogeneous elastic incompressible or compressible globe*, and for this assumed globe he has substituted the term *earth*, the same as the algebraist uses the terms x and y.

Is there a single geologist who believes that it is possible for this earth to have any such structure as that assumed for it by Thomson, Darwin and others? The difficulties placed by the physicists in the way of a belief in the earth's liquid interior, seem to be of their own making. These difficulties arise from the assumptions and limitations that the physicists have to impose upon the problem to bring it within the range of mathematical analysis. They have taken premises that no geologist would take, and having proved their point regarding these assumed premises, then claimed that they have proved it for this *earth*. What physicist has taken as his basis the most probable condition of the earth if its interior is liquid; a heterogeneous, viscid, elastic, liquid interior, irregularly inter-locked with and gradually passing into a lighter heterogeneous crust? The problem is believed to be beyond the power of any transcendental mathematics now known, and it is not believed to be possible mathematically to prove, at present, anything regarding the actual state of the earth's interior.

Our conclusions as to that state appear to be dependent on the evidence that can be derived from geological and petrographical studies. Professor Hennessy appears to have taken, as the basis for his mathematical discussion, data that are nearer the probable constitution of the earth than those assumed by any one else, and his results are entirely consonant with the hypothesis of a fluid interior.

It is as necessary that physical and mathematical discussions of the state of the earth's interior should conform to geological facts, as it is that geological theories should conform to physical and

[1] Philos. Trans., 1882, CLXXII, 187-230.

mathematical laws. It is incumbent on the physicist to explain earthquake motion, the sinking and rising of different portions of the earth's crust, volcanic phenomena, the uniformity in composition of lavas, the structure of volcanic rocks, sedimentation, faulting, vein formation, etc., etc., by his theory of a solid globe. Geological *facts* are just as positive as physical ones, and it is as necessary for the physicist to reconstruct his theory of a solid earth to suit geological facts, as it is for geologists to reconstruct their theories to suit the so-called physical demonstrations of a solid earth—demonstrations that merely show that under the assumed conditions and hypotheses the physicist's imaginary globe must be a solid one.

Are not speculations, speculations still, even if threaded together by a long series of mathematical formulæ, and are not all so-called mathematical proofs of the earth's solidity the mere working out of certain assumptions? Huxley was indeed right when he said, as the present writer would like to say: "I do not presume to throw the slightest doubt upon the accuracy of any of the calculations made by such distinguished mathematicians as those who have made the suggestions I have cited, * * * but I desire to point out that this seems to be one of the many cases in which the admitted accuracy of mathematical processes is allowed to throw a wholly inadmissible appearance of authority over the results obtained by them. Mathematics may be compared to a mill of exquisite workmanship, which grinds you stuff of any degree of fineness; but, nevertheless, what you get depends on what you put in; and as the grandest mill in the world will not extract wheat-flour from peascods, so pages of formulæ will not get a definite result out of loose data."[1]

Not only in mathematical and physical science, but in all science, no amount of discussion can give the conclusion any greater strength than the premises have, but attracted by the conclusion or by the chain of argument, the data are apt to be overlooked. Were this not the case many a structure, reared with great care and now regarded as established, would be found to rest on flimsy foundations.

So far then as the mathematical and astronomical determinations of the earth's solidity are concerned, it would seem that great doubt exists as to the correctness of the premises as applied to the earth, and hence like doubt regarding the conclusions.

[1] Presidential address. Geol. Soc. London, 1869, p. l.

When calculations shall be based upon the best data obtainable as to the materials of the earth's interior and their arrangement therein, then, and not till then, will mathematical calculations have real weight that geologists are bound to respect, but that day is a far distant one.

In the mathematical discussion of the state of the earth's interior, it has been customary to select a homogeneous globe of uniform density, but when the earth has been assumed to be heterogeneous and of varying density, it has been regarded as composed of layers, the layers being of unequal density when compared with one another, but of equal density throughout their own mass.

The more probable condition is that of a globe with a gradual increasing density from the exterior towards the interior, but with the materials heterogeneously arranged in the so-called layers. While the heavier materials would increase in abundance and the lighter diminish as the interior was approached, and *vice versa*, these materials at any one point would be found to be mixed together in varying proportions, the same as they are in meteorites and terrestrial eruptive rocks. While the extremes of the series may, indeed, be free from one another's components, the intermediate layers would contain more or less of the materials that predominate in the other portions of the earth's interior.

(To be continued.)

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VESTIGES OF GLACIAL MAN IN MINNESOTA.

BY MISS FRANC E. BABBITT.

DURING the open winter of 1878–9 and the seasons immediately succeeding it, the writer of this paper chanced to be engaged in searching certain terrace surfaces of the Mississippi river for palæolithic remains supposed to exist at the locality under examination. Rudely worked quartzes had previously been discovered here by the State geologist of Minnesota, Professor N. H. Winchell, by whom they had subsequently been described and figured in the State geological report for 1878. The field of exploration lies in central Minnesota, about one hundred miles northwest of St. Paul, and within the township and small village of Little Falls, Morrison county. Its area is at present restricted